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THE
PSYCHOLOGICAL BULLETIN

THE PSYCHOLOGICAL EFFECTS OF DRUGS

BY MAX F. MEYER

The University of Missouri

It is no easy matter to sum up the work on drug effects done during the last decennial period. The difficulty does not result from there being so large a number of studies. The number is not excessive. But it is difficult to summarize because so few of those who report results seem to have conceived their problem in definite terms. In many cases one gets the impression that the author was induced to make an experimental study, not by the conception of a problem, but rather by the fortuitous fact that a certain drug existed. Such studies, unguided by theoretical forethought, are, of course, capable of yielding valuable discoveries. But they are more likely to yield merely some records which their author can give the appearance of having scientific value only by searching in text-book psychology for a chapter heading in traditional terminology fit to receive those records as its children,—as when one of our authors reports that bromides reduce “the power of attention.” Has that statement any scientific meaning?

No reference will be made in the following to articles which in the present writer's judgment interest exclusively a surgeon in quest of health for his patient, or only a physiologist working on problems of the entirely unsocial life of the individual, or only a sociologist arguing for or against drug prohibition. Of the greatest value have been to the writer the previous summaries on drug effects published in the BULLETIN by A. T. Poffenberger in 1914, 1916 and 1917.

Many psychologists most naturally are interested especially in the question how those drugs which our social habits tempt us

to apply to ourselves increase or diminish our efficiency as workers of whom human society expects a certain output of work and rewards us or punishes us according as we accomplish more or less than is expected of us.

It is natural enough that during the last decade alcohol should have interested a larger number of students of the drug problems than any other drug. Prohibition was coming and came. Nevertheless, the psychological studies here reviewed furnish virtually no argument either for or against prohibition. Even if such studies show that a drug makes us weak, it is still an open sociological and moral question if it is not perhaps desirable that at times we should be weak.

The chief interest of the psychologist may be summed up in the question whether smaller or larger doses of alcohol during the succeeding moments of time increasingly or decreasingly affect by weakening or strengthening equally or unequally the lower and the higher centers of the nervous system accustomed or unaccustomed to alcohol. To this question we receive the following answers:

1. That the nervous system is the less affected by any given dose of alcohol the more it has previously been subjected to this poison is so generally agreed on by all those who mention this phase of the question, that it is unnecessary to call the witnesses by name. Pierre Janet (13), in connection with this fact, makes a statement, however, which to the present writer seems misplaced. After mentioning the fact, identical with the one just referred to, that "drunkenness" is not a characteristic of the alcoholic, he adds that states of very great "mental depression" are frequent in alcoholics. Asserting now, introspectively, that mental depression is equivalent to "terrible suffering," he makes the following plea: "If we recollect that alcohol rescues alcoholics from terrible suffering, we shall understand that it involves for them temptations that a normal person does not feel."

The present writer, who is virtually a lifelong total abstainer, asserts that he also has quite frequently (Maybe he is not normal, but who is?) states of very great "mental depression" due to such causes (other than alcoholism and also mentioned by Janet) as "overwork, too great ambition, struggle." He dares Mr. Janet to prove that the present writer's (introspective) "terrible suffering" is less than the alcoholic's (introspective) "terrible suffering." The only objective difference seems to be that the present writer,

although he has quite enough experience with alcohol to know how he could easily rescue himself from his "terrible suffering," *does not have the habit* of thus rescuing himself, whereas the *average alcoholic has acquired that very foolish habit*. Does not the difference in *intelligence level* offer a more (among various additional ones) acceptable explanation of the possession of, or freedom from, such a foolish habit, than the supposition of *different quantities* of a purely *subjective* and therefore not measurable "terrible suffering and temptation"?

2. That alcohol, to a *superficial* and easily illusioned observer (who may be the very person who took the alcohol), changes a person from being less reactive to being more reactive, that is (shifting the system of coördinates) from more depression to less depression, is so generally agreed on, that this fact, too, does not call for witnesses named among those reporting their experiments. But this is entirely in agreement with the fact that the drug *weakens* certain (which, we do not know yet) functional properties of the nervous system. The only outward weakening which the *superficial* interpreter regards as plain weakening, however, is unfortunately that of the drugged person's "acting sleepy." But the drugged person's "acting lively," when that really consists in no more than making puns, being clownish or considering all risks of life as having vanished, is not the logical opposite of "acting weakened, sleepy." Is it not a "weakening," too, when the *best* reactions a drugged person can make consist in repeating the stimulus word, or saying nonsense, or being wordy? Compare Jörger (14) and Miles (22).

It would add much to the clearness of expression if psychologists would agree to speak of the "weakening" effect of alcohol and to avoid entirely the misleading term "depressing," however *popular* the latter may be. And still better reasons do psychologists have for never formulating such a question as that of the "depressing or *stimulating*" effect of a drug, for a psychologist ought never to speak of *stimulation* unless he has in mind a stimulus acting on a sense organ. Why not formulate the question by using the terms "*weakening and strengthening* a certain functional property of the nervous tissue" rather than "depressing and stimulating"?

3. There seems to be little doubt that the difference between the effects of small and of large doses of alcohol is nothing but the difference between little and more effect of the same kind

on the same tissues. Some authors state this very emphatically. "Variation in size of dose causes only quantitative change," Lange and Sprech (17). "The larger the dose, the greater is the effect," Karlson (15).

4. The effect of the alcohol on the nervous system reaches a maximum about 90 or 100 minutes after imbibing (Dodge and Benedict (6), and Benedict (2)).

No sufficient light seems to have been shed during the work of the last decade on the following questions: Does the time when the maximum effect is reached differ with the size of the dose? It is quite possible, for example, that a small dose will reach its maximum effect sooner than a large dose. Does the effect increase first rapidly and then slowly during the interval between imbibing and the maximum effect, or the reverse? And how about the manner in which the effect decreases with further time? Here are large open questions for the future investigator.

5. Whether the alcohol dose affects the higher and the lower centers of the nervous system equally or unequally is a question of enormous significance. The consensus of opinion seems to be that the function of the higher centers is more weakened than that of the lower centers. But the fact is expressed in various kinds of phraseology. Benedict apologizes for the apparent observation to the contrary by introducing the mysterious stranger of an "autogenic reënforcement" which causes such an illusory observation. The present writer would put this autogenic reënforcement in the same class with such phenomena as a person not being able to do good work in the morning until he has *strengthened* himself by eating breakfast. Likewise, some people may *strengthen* themselves by a dose of alcohol. Rivers, for example, found typewriting speed increased. Others do it by silent, or loud, prayer.

Karlson states that "alcohol impairs every faculty; the higher the faculty, the greater is the effect." Karlson's statement, that the impairment is one of quality rather than of quantity of work, seems to have the same meaning. Does not the quality depend on higher centers more than the quantity? And the rats of Bagg (1) who did not get out of the maze very easily after having inhaled alcohol fumes probably used in this work the very highest centers which Nature had placed at their disposal.

The chief deficiency of the experimental investigations concerning the difference of the drug effects on higher and lower

centers consists in the fact that really high centers in the human nervous system have been systematically avoided by the experimenters. This is true not only for the alcohol investigations, but for those of all other drugs. But we may as well point out here at once this general deficiency with regard to alcohol.

Dodge and Benedict, for example, have intentionally selected for their observations "processes as remote as possible from voluntary, conscious modification and control." The idea obviously was a double one: first, to study functions as simple and primitive as possible, served by as low centers as possible; and secondly, to exclude the trouble caused by the learning process, which prevents the measurements, even without changing the dose of the drug, from being constant. So much, so good, as far as the lower centers are concerned. But with the higher centers the idea would again be a double one: first, to study functions as complex as possible, served by the very highest centers; and secondly, to exclude again the learning process.

That, however, is obviously impossible. There are no high intellectual functions which are not subject to the learning process, that is, to improvement from case to case. It does not do any good to select as highly intellectual functions such functions as typewriting and multiplication. Such functions as the use of the multiplication table are not, in human life, to be considered as highly intellectual. And the worst is, that even these are still subject to the learning process. Some investigators have tried to overcome this last-mentioned difficulty by making the subjects first go through a "preliminary practice." But they will not find anybody credulous enough to believe that that preliminary practice really abolished all further learning.

The present writer believes that one should bravely face the enemy, that is, the learning process. Indeed, instead of avoiding him one ought to invite him to coöperate. If one wants to discover the influence of drugs on intellectual processes, one ought to select the very highest intellectual processes, provided only that one can measure them. How to get rid of the learning curve is a separate problem. The present writer believes that that problem is easily solved.

He is using in experiments now in progress a simple method which seems to be capable of solving it. But the experiments will not be published until more data have been accumulated, and he will therefore here abstain from counting his chickens

before they are hatched. He merely wants to emphasize that in his opinion one serious deficiency of the work done during the last decade consists in avoiding all those processes which are subject to improvement during continued testing. They are the very processes that interest us most. An artist drinking a bottle of wine before going to work does not intend thereby to improve his knee jerk. And the student who drinks a cup of tea before a mathematics examination does not intend thereby to improve his use of the multiplication table. The highest intellectual processes ought to receive the chief attention of the experimenters of the future.

So far as tobacco is concerned, our present knowledge is much less satisfactory than that with regard to alcohol. Berry (3) and Bush (5) report absolutely contradictory results. The former reports that on smoke days the work was done in less time and with fewer errors. The latter reports a decrease in efficiency. The present writer, on the basis of his (unpublished) experimental data, believes that Berry is right and Bush is wrong, so far as the real drug effect of the smoke is in question. Here as with alcohol the "autogenic reinforcement" (whatever that may be; call it suggestion, if you wish) is a very disturbing factor in experiments. But in standing with Berry, the writer does not wish to disagree with C. K. Taylor (24), who brings forward plenty of evidence that smoking is an evil, especially for those who are not yet fully grown. And the same view is suggested by W. H. Burnham (4), even though he points out that the fact of smoker boys being of inferior scholarship is likely to be due to a third factor as a common cause, namely, social inclination.

A good deal has been learned about caffeine and its relatives. C. K. Taylor (25) tells us the very interesting fact that coffee-drinking children in the schools (like smoker boys) are not likely to be found among the physically and mentally superior children. But he does not prove that they have been stunted exclusively or chiefly or in any degree by drinking coffee. He fails to emphasize that coffee-drinking children are quite likely to have inferior parents. Frankfurter (7) reports increased efficiency in type-writing resulting from tea and caffeine. The elaborate work of H. L. Hollingworth (1) is in full agreement with this.

Raising the same five questions which we raised with respect to alcohol, the answers would be the following: (1) General addiction or abstinence from tea or coffee seems to make no appre-

ciable difference in the effect of a dose. This condition is thus very different from that of alcohol. It does make a difference, however, whether the stomach is empty or full. And a given dose has less effect on a heavier body. (2) The effect is a strengthening of certain functional properties of the nervous system. (3) The effect increases with the dose. An overlarge dose, however, is likely, after a short time of increased efficiency, to result in decreased efficiency. This seems to be causally connected with the fact well established by physiological investigators during the last decade, that large doses of caffeine have a harmful effect on many different kinds of tissues of the animal body. (4) The effect increases during the first hour, is still noticeable after four hours with little decrease and may last quite a while longer. This has some significance on its effect on sleep. A very small dose, however, is not likely to interfere with sleep. The exact rise and fall of the time curve of this drug is a problem of the future. The positive effect, however, does not seem to be followed at any time by a negative reaction. (5) The drug seems, like alcohol, to affect the higher centers more than the lower ones. Further evidence is still needed.

Schilling (27), in what is probably the most recent experimental contribution, finds in reaction time tests always continued for an hour that the reaction time during that hour becomes longer and longer, and more so after imbibing caffeine or acetanilid than after imbibing a mere "control". The reviewer thinks that a plausible way of interpreting the result of Schilling very briefly, and therefore in very popular language, would be that of saying: The caffeine *increased the boredom or impatience* incident to the experimental procedure and therefore increased the reaction time through causing loss of interest in its being measured.

Strychnin is one of those drugs which are not offered to us in the grocery store. Nevertheless, it interests the psychologist because the answer obtained for the above five questions may help him to decide whether those five questions are well formulated. (1) Nothing seems to be known about the influence of previous addiction. (2) The effect seems to be a strengthening of a functional property of the nervous tissue. But speaking of human efficiency in general, Poffenberger teaches that moderate doses of strychnin taken into the stomach produce no clear-cut change. (3) As to the difference in effect between large and small doses,—when Lashley (18) tells us that a *large* dose facilitates (in an albino rat) the learning of a maze, whereas caffeine retards the learning,

we ought to consider what was said above under (3) about large doses of caffeine. (4) Nothing seems to be known yet about the time curve of strychnin, except that Lashley reports the absence of serious after effects. (5) Strychnin seems to affect the lower centers especially, whereas atropin seems to hold the middle ground between strychnin and caffeine (Poff. 14). Porter (23) tells us that strychnin tends to bring all reflexes to the same *maximum* speed, and that therefore it has a less conspicuous influence on those reflexes which, like the flexion reflexes of the cat, have already without the drug a great speed.

About another drug, mescaline, Knauer and Maloney (16) say that it produces "hallucinations," a state somewhat midway between the hypnotic and the normal state. When they say, however, that immediately preceding experiences have some influence on the nature of the hallucinations, one wonders what that remark has to do with the drug.

Meier (21) finds that bromides have the effect of weakening the functional properties of the nervous tissue. Certain results might be interpreted by the conclusion that the weakening is stronger in the higher centers.

V. H. Veley and W. L. Symes (26) worked with stovaine and various homologs. One might conclude from their results that this drug has a greater effect of weakening, though acting more slowly, on the lower centers than on the higher centers.

The work of S. I. Franz and W. C. Ruediger (8) with ethyl chloride as an analgesic interests us especially because it shows that the hairs have two distinct sensory end organs, though possibly no separate nerve supply. The disappearance of the touch sensation is less persistent than that of the pain sensation.

Opium alkaloids according to Macht and Isaacs (19) seem to have on the nervous tissue first a brief effect of strengthening and then a very long effect of weakening. The larger the dose, the more abbreviated is that first effect of strengthening certain functional properties.

Goddard (9) showed that pineal extract has no effect on "mental development" in human beings. McCord (20) asserted that it increased "mental development" in dogs, though not in guinea pigs and chickens.

Friedrich Hacker (10) studied the effect of antikenotoxin. Antikenotoxin is a commercial chemical recommended against fatigue, first prepared by Weichardt and sold by Kalle in Biebrich. Kenotoxin is the name given by Weichardt to those substances

(some of them produced by the active animal organism) which produce in the animal's muscles, nervous tissue, and possibly other tissues, the physiological phenomena of "fatigue." These kenotoxins are far more complex compounds than such compounds as lactic acid or uric acid. Previous experimenters assert a fatigue-retarding effect both on muscle work and on scholarly work, after either subcutaneous application of antikenotoxin or inhaling it sprayed in the air. Hacker found that the continuous reading of nonsense syllables was not aided through antikenotoxin, whereas it was conspicuously aided through caffeine. The same result was obtained in muscular work under antikenotoxin or caffeine. Arithmetical school work under antikenotoxin sprayed in the air appeared entirely uninfluenced.

With respect to the question of "consciousness" the present writer would divide psychologists into two classes: those who believe that an analysis of their own (they cannot analyze any other) consciousness might (but not must) guide them in their scientific pursuit of objective facts and theories; and those who believe that it is the task of the psychologist, or at least one of his tasks, to find an answer to the question whether this or that concrete individual in this or that concrete situation has this or that consciousness or none. E. Jacobson (12) seems to belong to the second class. He decides his question to some extent by stating as his conclusion that persons are conscious under anesthetics even during the height of a surgical operation. His conclusion is based chiefly on his "memory" afterwards. He adds, quite rightly, that "amnesia" after the operation can never be regarded as proving that during the operation there was *no* consciousness. Those interested in metaphysics may decide whether "memory" after the operation is capable (as Jacobson thinks) of proving that during the operation there *was* consciousness.

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FEELING AND EMOTION

BY ARTHUR I. GATES

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THE EMOTIONS—THEORETICAL

Link (13) is convinced that "the classical concepts of emotions and instincts are a hindrance rather than a help to future studies." By regarding the emotion as a mental state and the instinct as the bodily change which characterizes the emotion, "it is made absolutely impossible to establish any causal identity, or even a schematic identity between the two." For example, experiments with animals are quite futile because it is impossible to secure data on the psychic phenomena. The classifications of emotions given by McDougall and others are said to be entirely arbitrary and misleading. If emotions are, as McDougall believes, fixed and independent in character, it is impossible to account for their fusion into "sentiments" which are more powerful than the fundamental forces themselves; "only the most fanciful and uncritical imagination can follow the process by which it is done."

Kantor (11), none the less destructive, is more constructive in his two articles. He submits that the emotion is not a positive response to a stimulus, but rather a failure of a stimulus-response coördination to operate. Emotions are essentially "no response" activities. The individual left without a directed mode of adjustment is thrown back upon primary responses, namely, the organic reflexes. "It is these *replacement*¹ reflexes which give emotional conduct the appearance of positive adjustment."

Several consequences must follow from such a negative character of emotions as Kantor describes. 1. Emotional activity can be of no general and necessary utility to the organism; in fact, emotional conduct must be always truncated and ineffectual action, "disrupting chaos," momentary paralysis. 2. Emotional conduct must not be interpreted as an hereditary form of behavior, since it is due to the breakdown of acquired stimulus-response systems or to the absence of adaptive reactions. 3. Because emotions are "no response" actions, they cannot readily be clas-

¹ Italics mine.

sified. Each must be studied as it occurs, in connection with its particular stimulus-response conditions. Most misleading has been the custom of connecting emotions with "those teleological entities called instincts." 4. It follows from the disruptive character of emotional behavior that emotions are seldom, if ever, observed in animals and young children, since such organisms have not reached the stage of acquiring sufficient response systems to become disrupted. Kantor urges the discard of the search for biological utility for a direct study of emotions as responses to "disrupting conditions of the environment."

Shand (22) has restated objections to McDougall's theory of the association of specific primary instincts with specific emotions. The emotion is a general activity, including an impulse, aroused when an instinctive impulse is not leading successfully toward its consummation.

Differing from all these hypotheses, and in most respects the very antitheses of Kantor's, are those appearing in Woodworth's recent *Psychology* (26). While the emotion is, introspectively, a "stirred-up state of mind," suggesting a breakdown of the organism's integrations, as a matter of fact, the "emotion represents internal preparation for some type of over action." It is pointed out that although sustaining evidence of utility is lacking in the case of many emotional states, there is no noteworthy contradictory evidence. For example, the fact that certain emotions, under civilized conditions, may be of no utility, does not discredit the theory since it is based on the assumption of adaptation to primitive environment, and not perfect adaptation at that.

Woodworth makes good use of the James-Lange theory without accepting it wholly. The trouble, in his opinion, has been a confusion of emotions with impulses, the latter being really the criteria by which emotions have been differentiated in speech. Fear and anger, as organic states, are much alike, but as impulses quite unlike. Typically, the impulse generates the emotion, but the emotion is never the same as the impulse. Temporarily the order of events is: *Stimulus*, say, a bear; *Response*, (a) seeing the bear; (b) recognizing a dangerous situation; (c) adjustment toward escape (i.e., impulse); (d) internal preparatory reactions, glands, visceral mechanisms, etc.; (e) conscious stirred-up state consisting of blended sensations of all these preparatory reactions (i.e., emotions); (f) definite escape reaction.

Emotions are "native states of mind; or, as modes of behavior, they are like instincts in being native behavior." Like McDougall

he posits certain primary emotions, such as anger, fear, lust, "the comfortable state appropriate to digestion," grief, mirth, disgust, curiosity, the tender emotion and "probably a few others." The really distinct primary emotions are much fewer than the instincts, but "several of the primary emotions are attached to specific instincts." While this sounds like McDougall, the theories are essentially different; the presence of a primary emotion is not, for Woodworth, the criterion of the primary instinct. The important relationship is merely that in some cases the emotion represents bodily readiness for instinctive action. Woodworth's theory is thus an adaptation of the hypotheses of James, McDougall and Cannon, with certain features found in none of them.

McDougall's analysis of belief (16) leads him to include it with confidence, hope, anxiety, etc., which are treated in the *Social Psychology* as "derived emotions." All of these are members of or named points in a continuous scale of emotional experiences which may accompany and qualify the operation of any strong desire. Belief is very similar to confidence.

Larguier des Bancel (12) finds the emotional chill experienced by the observer of art, music, etc., to be a residual of primitive fear whose stimulus was the mysterious. The chill may appear in admiration, since the latter is a compound of curiosity, humility, and fear.

Janet (9) has submitted as a general explanation of phobias, the "fear of action." The phobia is an alibi or defense mechanism which enables the subject to avoid some unpleasant work or activity. Kaiser (10) has discovered a new generalization which is alleged to include all springs of action. He finds that a "craving for thrill" is the fundamental instinct, and that all varieties of activity are its results. The interesting implication of this paper is the convenience with which "sex" might be substituted for "thrill," or how, in Freud's writings, the reverse might be done without interfering with the underlying logic of either system.

Buscaino (4) describes a number of clinical cases portraying disturbed emotionality correlated with defects of the mid-brain, particularly the thalamus. He adopts a theory, essentially the James-Lange, which, he finds as did Titchener earlier, has been repeatedly framed from the time of Aristotle.

The new and admirable text on the form and function of the central nervous system, by Tilney and Riley (23), includes frequent generalizations concerning the localization and dynamics of

emotions and feelings. The belief that the frontal lobe dominates all behavioral reactions is reminiscent of Wundt. The rich connections of the frontal lobes with the thalamus provides for a great expansion of feeling tone "which plays such a compelling part in the motives of all voluntary activity." All of the superstructures posited by these writers are clearly patterned after the theories of McDougall. For example, "the primitive emotions enter into secondary and tertiary combinations, and thus determine the more complex emotions and sentiments. Yet however complex they may become, these psychic combinations of the cerebral cortex are fundamentally dependent upon the thalamus for their primitive source of affective energy." The reader who seeks the evidence of this dependence, or how it comes about, will be disappointed.

THE EMOTIONS—EXPERIMENTAL STUDIES

A study of emotional expressions by means of photographs reported by Ruckmich (20) showed, as did similar experiments by Langfeld, that the mood of the subject colors the interpretation. It was found that the lower half of the face provides more important clues than the upper, and that the mouth and eyes provide specially important signs.

Russell's selections (21) from the poetic writings of Browning provide interesting descriptions of emotional consciousness and expressions.

Ferrari (8) and Gualino (9) have described the behavior of several soldier deserters condemned to be shot. It was found that evidence of disintegration appeared earliest in the postural reflexes, mental control usually being maintained until the last.

The possibilities of detecting deception by the use of objective tests are optimistically described by Marston (15). While the galvanometric, association reaction-time and the Benussi breathing tests were found wanting, the systolic blood pressure records were reported to have given "100 per cent. accuracy of judgment under very different conditions." Burt (2, 3) has improved the Benussi inspiration-respiration ratio procedure for determining the lying consciousness and finds it a useful supplement to blood pressure methods. The latter he finds misleading in 10 per cent. of the cases.

An introspective and questionnaire study of the development of admiration by Moore (17) shows pronounced age differences,

characterized by a decline in admiration of relatives and religious characters, and an increased admiration of brave and powerful public characters. A great decrease in the relative numbers of fears and fear dreams between ages four and nine (from 80 to 12 per cent.) appeared in an investigation by Boyd (1). This writer believes that both specific and general fears are inherited, the criterion being the unlikelihood of opportunity for acquiring them. The instinctive fears have as stimuli, eyes, teeth, snakes, great wild animals, wind and sea and death.

Pressey (19) has offered a second revision of a series of four group tests of emotional traits. Basing his statement on the study of a hundred college students, the author says that "such examinations will be more accurate than Army Scale Alpha in prognosticating unsatisfactory work in college."

FEELINGS—THEORETICAL

Along with his criticisms of the classical concepts of the emotions, Link (13) finds equally great defects in the treatment of the affective processes. These, he finds, are not emotions, nor are they guides or results of emotions. The feelings are, in fact, the more fundamental, both genetically and actually, since they appear merely as symptoms of "getting along well" and "getting along badly." Since feelings, however, imply a normal course or "possibly a purpose," we must look still deeper for the fundamentally dynamic factors of behavior.

Kantor (11) believes that his naturalistic theory of emotions (the emotion being a collapse of situation-response adjustment) provides the setting for a precise distinction between these phenomena and feelings. The presence of an organized response system in the act is the criterion of feeling. Furthermore, feelings may be conceived as having potency to condition other activities, "in the sense that while they are operating they will affect any activity the person is performing," whereas the emotion, being merely the replacement of organized responses by a chaos of organic reflexes, has no such dynamic influence.

Woodworth (26) adopts the concept of simple feelings, *i.e.*, pleasantness and unpleasantness together with a neutral state of indifference. The simple feelings differ from sensations in that (1) they cannot be introspectively observed as sensations may; (2) they cannot be localized as sensations may; and (3) they have no definite sense organs. They are not considered to be associated

with definite organic activities, because of the overlapping of states of pleasantness and unpleasantness; nor with differences in the ease of brain action, since the facts of practice would contradict such an hypothesis. The fact that feelings are associated with impulses is at the core of the whole problem. Pleasantness goes with a neural adjustment directed toward keeping . . . while unpleasantness goes with an adjustment toward riddance. The nearest approach to a statement of cause is this: "Bitter is unpleasant because we are so organized, by native constitution, as to make the riddance adjustment on receiving this particular stimulus. In plain language, we seek to be rid of it, and that is the same as saying it is unpleasant. Sweet is pleasant for a similar reason. In indifference there is no tendency either to keep or to be rid of it."

There are, however, two kinds of feelings: (1) those typified by sweet and bitter which are immediately and invariably aroused and (2) those which may be called secondary because they depend on "pre-aroused desires," *i.e.*, eating, fighting, catching a train. "Just arouse any desire, and then you can give pleasure by gratifying it, displeasure by thwarting it." There are, also, *native* likes and dislikes, such as those for mathematics, music, colors, which may be contrasted with *acquired* likes and dislikes, such as those for cheese, tobacco or color combinations. Woodworth contends in opposition to McDougall that the likes and dislikes for mathematics, odors, machinery, etc., are not necessarily dependent upon instincts but are primary.

Duprat (6) suggests as a basis for the practical classification of men, the expansive and depressive types. Starting from some of the data on feeling, this writer is soon generalizing far beyond experimental limits.

FEELINGS—EXPERIMENTAL STUDIES

An experimental study directed to the fundamental issue of the validity of feeling as a unique, non-sensory element as Titchener contends, was reported by Yokoyama (28) who employed the method of paired comparisons. The introspections of this writer's subjects indicate that pleasantness and unpleasantness are meanings or attitudes for which organic sensory content is the *sine qua non*, at least under the conditions of the paired comparison procedure.

In Young's investigation of feelings aroused by odors, tastes, etc. (29), unpleasantness was found to be associated with reactions away from the stimulus-object, whereas pleasantness was organically-kinæsthetically negative,—the subject did nothing. Corwin (5) believes that these results were due to the conditions of the experiment which made pursuit unnecessary under pleasantness. With certain changes in the setting, Corwin found that pleasantness correlated with actual pursuit, tendencies to pursuit, or a kind of expansion or relaxation *toward* the stimulus-object.

Color and form appear to act in independence in determining the affective response to a combined color-form, according to Yokoyama (28). The affective tendency of the color-form unit varies approximately with the algebraic sum of the affective tendencies of the constituent color and form. This finding is essentially in accord with the orthodox conception of the simplicity of feeling tones, as, for example, that given by Geissler in the Titchener Commemorative volume: "the greater the pleasantness of the individual constituents, the greater will be the pleasantness of the combination." M. F. Washburn (24), using the method of judgments of single stimuli, reaches a different conclusion, that the effect of the combination is by no means the summation of the effects of the elements. Frequently two colors, individually pleasant, form an unpleasant combination. Washburn and Grose (25) found it possible to change the affective value of color by four devices: (1) ordinary adaptation (affective fatigue); (2) shifting of attention to various constituent features; (3) imagining different contexts; and (4) deliberately setting up the contrary reaction (compensation). The method of imaginary context was most frequently and successfully employed by the subjects. Extreme judgments were harder to change than moderate; extreme unpleasantness was harder to change than extreme pleasantness.

The emotional significance of various lines (drawn by pen) was studied by Lundholm (14). For example, beauty is expressed by unity of direction, continuity, roundness of curves, lack of angles and a certain symmetry; ugliness by the reverse of these. Anger, sorrow, etc., are suggested by various combinations of these factors.

An extensive study of the dynamic effects of colors was undertaken by Pressey (19). Performances in tapping, multiplying, reaction-time, etc., were not perceptibly influenced by either color or light intensity. Introspective estimates of feeling attitudes

varied from subject to subject with agreement that the affective value became less with habituation. Reviewing the literature, Pressey feels that there is no reliable evidence that hues have a marked effect either upon emotional tone or on ability in mental work.

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THE PSYCHOLOGY OF VOCATIONAL SELECTION

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The term vocational selection may be interpreted very broadly or very narrowly. It is sometimes made to include a large part of the vocational guidance field and, on occasion, the fields of educational selection and guidance as well, while at other times it is limited strictly to the matter of choosing men to fill particular jobs. The present review makes use of the more limited definition. We exclude vocational guidance and placement (selecting jobs for individuals in contrast with selecting individuals for jobs), educational guidance and selection (both the selecting of courses of training for individuals and the selecting of individuals for courses or classes), physical examinations for selection, and all the wealth of mental test material where the tests have not been either devised for purposes of vocational selection or actually used for such purposes.¹

Vocational selection is a practical affair. True, certain of its methods and tools are distinctly psychological. But any decision as to what may and what may not be labelled the "psychology" of vocational selection must be regrettably arbitrary. The close interrelation existing between applications and "pure" science in this field, or better, between administrative and research activities, bears upon the nature of the present review in several particulars. In the first place, the dependence of the scientific aspects of vocational selection upon the actual progress of employment procedure makes it desirable to sketch, in barest outline, the developments in industrial employment practice before we proceed to a more detailed discussion of the work that has been done on separate phases of the employment problem. Secondly, it is to be noticed that since the literature dealing with employment

¹ In addition to these topics specifically excluded, a large number of references are omitted which contain borderline material or material that was considered relatively unimportant for the present purpose. There have doubtless also occurred certain unintentional omissions of articles that should properly have a place. Especially as regards contributions not written in English, the review makes no attempt at completeness.

is almost boundless—much of it composed of popular and semi-popular discussions or reports of procedure—no attempt is made to include a large part of it. Rather the policy has been to mention a few of the more representative and significant discussions on the practical side of the several topics treated.

RECENT HISTORY AND PRESENT STATUS OF VOCATIONAL SELECTION IN INDUSTRY

Employment methods have been undergoing a profound change during the last decade, a change which is best summarized in the expression "functionalized and centralized employment". Scientific management, with its emphasis upon specialization and the separation of the various managerial functions, paved the way for the entrance of functionalized employment. The hiring and firing activities which were formerly the prerogative of individual foremen have, in an increasing number of plants, been taken out of the hands of foremen and concentrated under a staff department which deals exclusively with personnel duties (of which hiring is usually the most important). Centralized hiring necessitates the development of two bodies of basic knowledge—a knowledge of the requirements of the tasks for which men are being selected and a knowledge of the equipment of the individual worker. In the days of foreman hiring the need for written standardized job specifications was not felt, nor was the need for technical devices for determining men's abilities. Each foreman knew at first hand the jobs for which he was selecting men. Likewise he could crudely ascertain the applicant's trade skill by asking questions or, as was more usual, by "trying him out". Since records of turnover and employment costs were not kept and since labor was ordinarily plentiful, it mattered little how effective the selection procedure proved. All this changes when costs and records are kept, when one centralized department is responsible for efficient selection, and when the importance of having "the right man in the right place" has once been appreciated.

Centralized employment, then, has emphasized the value of knowing the requirements of jobs and knowing how to determine the qualifications of men. The scientific (may we say psychological?) attack upon this twofold problem, so far as vocational selection is concerned, is scarcely a decade old. The pioneer book in the field was Münsterberg's *Psychology and Industrial Efficiency* (99) which appeared in 1913. (Substantially the same book ap-

peared in German the preceding year (98.) The problems of vocational selection, it is true, had been prominent before this time among engineers in the field of scientific management (37, 49, 156), in the vocational guidance literature (20, 100, 112, 126), and within some few industrial plants. A small group of employment managers had begun meeting in Boston as early as 1910. But in none of these quarters had an effective effort been made to deal with the problems of vocational selection in any careful and scientific manner. (One exception is on record in the work of Thompson as reported by Taylor (156).)

Aside from Münsterberg's book, probably the most important one for the development of a "psychology of vocational selection" that appeared before the war was Blackford and Newcomb's *The Job, The Man, The Boss* (17). In spite of the unscientific character analysis methods it included, this book proved distinctly valuable in that it drew attention to the selection problem as it appears in its entirety to the employer, with healthy emphasis upon the human side of scientific management and upon individual differences and their utilization.

During the war remarkable progress was made in meeting selection problems both in the armies and in industry, principally though by no means exclusively in the United States. The direction of progress in the industrial use of psychological methods of selection is illustrated by a comparison of Münsterberg (99) with Hollingworth (59) and still more recently and strikingly with Link (83). The development is well reported in the committee reports of the National Association of Corporation Schools (104, 105, 106, 107, 108). More and more emphasis is placed upon the use of quantitative methods, upon the importance of the job analysis side of the problem, and upon a broad consideration of employment policies and methods in relation to business management and labor problems. In the United States Army unparalleled advances were made (in no small measure by professional psychologists) all along the line of scientific selection—studies of occupational requirements, methods of interview, use of general intelligence tests, special vocational tests, trade proficiency tests, rating scales, and records of individual qualifications and progress. Valuable accounts of these achievements are to be found in official reports (117, 178) and in a number of special articles (14, 163, 180, 181, 182).

The personnel movement in American industry began its period of rapid growth about 1916, favored by the unusual expansion

of industry and the unprecedented mobility of labor during the following few years. Leaders in the labor management movement have consistently had as one, if not *the* one, of their major interests, the development of scientific methods of selecting workers. The nature of the research and administrative activities in the field of industrial personnel during these years is reflected in the Proceedings of the Employment Managers' Conferences (121, 122, 123, 124, 125) and in the annual reports of the National Association of Corporation Schools (104, 105, 106, 107, 108), in the papers collected by Bloomfield (18), and in such studies as Kelly's *Hiring the Worker* (68).

In the years immediately following the war personnel work continued its rapid expansion and many psychologists (as well as social workers, teachers, and preachers) entered industry as personnel experts, as consultants, or as students in pursuit of special researches. In part there has been an effort to bear to industry the torch of Army methods and in part the attempt has been to develop new tests and other employment devices. Much of this work has supplemented in a valuable way the practical procedures that had been developing in industry. The net result of the combined research and applied activities has been a decidedly significant body of material in scientific employment methods. The more important features of these industrial selection methods, bringing together the experience of the Army and of industry, are described in numerous special articles and in many of the recent books on employment and personnel, such as those of Tead and Metcalf (157), Shefferman (146), Frankel and Fleisher (43), Kelly (70), Simons (148), and, with more psychological emphasis, Link (83), Scott and Hayes (142), and Reilly (128).

In England during the last year or two a number of books have appeared in industrial psychology. Prominent among these are Muscio (101), Myers (102), Watts (171), Drever (35). Significant lectures and articles have also appeared by Burt (24), Pear (115), Watts (170, 172), and others. In the books cited, problems of selection do not play the disproportionately large part that has been true of the work of American psychologists in the industrial field. As regards selection, the English books have done little more than rewrite the American experiences in employment psychology from Münsterberg to Link. Watts (170, 172) and others have stressed the necessity for a broader analysis of men in selection than has been common, including a study of instinctive trends and of higher creative qualities.

In Germany, a group of psychologists have been tackling industrial problems in a thoroughgoing manner with their emphasis, however, more upon vocational guidance and training than upon selection. Some work, however, has been done on selection, tending to follow Münsterberg's precedent of using miniature representations of tasks and elementary analyses. A few of the more important studies and discussions that have been published are those of Moede (95, 96), Piorkowski (119, 120), Stern (152), Martens (90), and Lipmann and Stolzenberg (86). Brief notes on the recent work of industrial psychologists in Germany are contributed by Link (85) and Kitson (76).

Little work on selection methods appears to have been done in France, aside from valuable physiological researches and a few scattered studies such as that of Lahy (79).

During the past year in this country there has been comparatively slight progress in employment psychology as a direct result of the industrial conditions. "Selection psychologists" are suffering with other industrial groups during the depression in business. The wave of high labor turnover on which employment methods were wafted into prominence has broken and further development of hiring technique does not appear to flourish in the resultant spray. There can be little doubt, however, that the application of psychological methods to the selection of industrial workers has gained a permanent place in the increasingly important movement to regard the human element in business. A certain amount of "over-selling" of the possibilities of psychological selection must be lived down; a considerable body of unscientific work must be scrapped; a more vital grasp of the employment problem in its industrial setting must be obtained; a host of painstaking and thorough researches must be pushed to completion. The lean years of the business cycle, in other words, are but forcing a purification of the soul of industrial psychology. Recent months have brought real though not unadulterated blessings in the progress of scientific methods in vocational selection.

ANALYSIS OF THE REQUIREMENTS OF VOCATIONS

The foundations of job analysis for purposes of selection may be traced to two independent sources. On the one hand, the scientific management movement, with its devotion to increased efficiency, recognized the importance of knowing the requirements of the work in order that competent individuals might be selected

as workers. But no systematic attack upon job analysis is reported. The second source was that of vocational guidance. The early vocational bureaus developed discursive characterizations of the kind of individuals needed in various vocations but all these attempts were non-specific and, of course, not intended for vocational *selection*. Typical of these "vocational psychographs" are those of Parsons (112), Bloomfield (20), and many other workers, a number of whom are included in Part III of Bloomfield's readings (19). Ulrich (166) uses the same sort of method in describing the requirements for certain higher callings such as that of medicine. (The early work in vocational guidance and more recent advances are summarized by Brewer (22).) Münsterberg in his *Vocation and Learning* (100), although he uses an impressive psychological scaffolding for his occupational analyses, adds little to the preceding literature since he retains the loose and general qualitative descriptions. Schneider (135, 136) strikes a somewhat original chord by describing the requirements of jobs in a series of paired broad characteristics, as settled-roving, creative-imitative, etc., which, however, are open to the same criticisms of generality, non-quantitativeness, and questionable usefulness for selective purposes. Seashore (144) describes the detailed psychological abilities that seem to be required in singing but his method was not extended to industrial vocations. In *Psychology and Industrial Efficiency* (99), Münsterberg adds little to the discussions in the vocational guidance literature and confines himself in the main to the use of job analysis for test building purposes. In a later book (97) he omits all reference to the study of jobs.

The voices of these earlier writers come not from industrial plants but from vocational guidance offices, libraries, and arm-chairs. Nor was their work aimed at problems of selection, though it had a pronounced effect upon the later job analyses constructed more distinctly as aids in selection. Much of the literature is still compelled to speak in the same loose terms that were used by vocational counselors ten years ago (as is evident, for example, in a score of books and articles that give analyses of the requirements for salesmanship); there are still outcroppings of Münsterberg's tripartite division of requirements of jobs according to the thinking, feeling, and willing, categories; and Schneider's classification appears to be especially favored by later writers, though how these requirements are actually to be used in selection remains, to the reviewer, something of a mystery.

But the present status of job analysis is to be interpreted as due more definitely to another line of development. In a sense this second line is continuous with the scientific management influence already mentioned. We refer to the rise of centralized hiring in industrial plants. In the period preceding our entry into the war, studies of job requirements were beginning to be made in industry, aimed specifically at the selection problem. Blackford and Newcomb (17) view the analysis of jobs in definite relation to centralized employment procedure, although their actual analysis blank calls for general mental and physical requirements the determination of which has not been demonstrated to be possible. Among the early developments of job specifications in industry are those reported by Reilly (127, 129) and Burke (23), and among the somewhat later improvements in form, those of Hubbell (61) and Stearns (150). These men (and many others) succeeded in putting the descriptions of occupations into brief, fairly definite, systematic, and readily usable, form. The occupational description work in the United States Army (117, 155, 14) and the descriptions published by the United States Department of Labor (167) present important examples of the methods that were just budding in industry before 1917. More important, this work served as a tremendous advertising influence and example which led to much wider study of job requirements in industry.

Recent advances in methods and forms of occupational descriptions for hiring purposes are well summarized in a number of publications in the field of personnel work (70, 146, 157) and particularly in Meine (91), Link (83), reports of the National Association of Corporation Schools (107, 108) and of the Industrial Relations Association of America (63). The recent emphasis is decidedly upon the need for descriptions that are more definite and concise and standard; statements of requirements that are in quantitative and determinable terms; the analysis of requirements that are differentially characteristic of particular jobs. Descriptions of jobs for purposes of hiring have been strongly influenced by the admirably detailed and exact work in job study of Gilbreth (48), Merrick (92), Lichtner (81), and others. The desirability of quantitative and detailed statements of requirements is well illustrated by Kitson (77) using the example of the work of proofreading. Some writers, including Hollingworth (59) and Link (83), seem inclined to undervalue the importance of specifications other than those that can be stated in terms of test stand-

ards. In the literature of employment management, however, occupational descriptions are discussed primarily as aids to interviewers—memoranda to guide the interviewer in ascertaining qualifications and in describing opportunities to the applicant. Insofar as the required qualifications can be stated quantitatively as, for example, in test scores, this should be done, but a vast amount of additional essentially non-quantitative information exists which is and will, despite advances in test methods, continue to be an essential part of job specifications.

ANALYSIS OF THE QUALIFICATIONS OF APPLICANTS FOR WORK

The Employment Interview and Aids in Interviewing.—In vocational selection, not a great deal that can be called psychological has been done toward making more adequate the common-sense methods of interviewing applicants and interpreting the information obtainable from application forms, letters of recommendation, and the like. Numerous reports of current employment office practice in books and conference proceedings contain useful information on these matters—based, however, upon opinions of employment managers and not upon any scientific determinations. A number of forms and methods that have proved valuable are presented by Kelly (68), Reilly (128), and Shefferman (146), including application forms, letters of inquiry, and interviewers' rating blanks.

One of the first clear statements of interviewing methods is that of Huey (62) in which several notes are sounded that have echoed through the later literature—the desirability of placing the applicant at ease, the importance of studying his appearance and manner, drawing out his interests and desires, weighing the facts of his personal history, rating (even though crudely) his character qualifications, and acquainting him with the essential features of the position for which he is being considered. Kelly (69) and Jones (66) emphasize many of the same factors but with more systematized methods of procedure. Avery (7) presents some keen and thoroughly behavioristic observations by which to interpret the applicant's actions and the information on the application form, but most of these are personal reactions and of doubtful validity. Link (83) strongly emphasizes the desirability of the interviewer's assuming the applicant's point of view. Scott (138, 140, 141, 27) stresses the value of considering the applicant's previous experience and judgments concerning the applicant made

by himself, by others who have known him, and by the interviewer. Standardized forms and methods are presented (27, 140) for more reliably obtaining these records and judgments. Fish (39) enumerates a variety of characteristics that should be determined in the applicant but methods for these determinations are not presented, unfortunately. In fact, this problem of judging personal characteristics has been sadly neglected save by proponents of systems of character analysis. Scott, however, has made a helpful contribution in the interviewers' rating scale (27) which at least calls forth explicit opinions on a variety of characteristics in place of the usual unanalyzed snap judgment. Certain methods found valuable in selecting men for particular work in the Navy are discussed by Stearns (151) who begins with the point of view of a psychiatrist and takes a sound well-rounded view in his study of candidates. The most careful and scientific classification of information concerning candidates and of methods for estimating a man's worth and usefulness were developed by the Committee on Classification of Personnel in the Army (14, 117, 182). These methods have been adapted to many industrial plants.

On the side of negative criticism of ordinary interview methods there are a few brief studies. Scott presents evidence in his *Scientific Selection of Salesmen* (139) of the unreliability of ordinary interview estimates of abilities. He had six executives interview independently 36 applicants and rank these candidates in order as to their ability for sales work with the company. Rather striking discrepancies occurred. Similar material collected by Scott is described by Gowin (53). Hollingworth (57) presents much the same sort of evidence demonstrating the unreliability of "size-ups" based upon letters written by applicants in response to advertisements. Other difficulties and criticisms having to do with the judging of applicants are discussed in connection with character analysis methods and with the use of rating scales.

In general it is true that we are not yet beyond the common-sense rule-of-thumb stage in the sizing up of applicants. Scott's (141) words of five years ago are still true: "Strange as it may seem, no standard practice has been evolved for conducting the interview in such fundamental features as the length of time devoted to an interview, the method of conducting it, the points to be looked for in the application, the standard scale for weighing the applicant, and the method of recording the interview."

Systems of Character Analysis.—Selecting men for jobs is a practical and urgent matter. Where scientific methods cannot

be used, common-sense methods and pseudo-science will be—and are. The interviewer, selecting men, desires to know something of the applicant's intellectual and physical capacities, his degree of proficiency in special lines, and also his moral characteristics, his volitional and non-intellectual traits. In the first two directions psychology has at least a method and a promise. As regards analysis of character and temperament, scientific psychology has perforce remained silent, except where it has been aggressively negative. In a few quarters, however, the problems are being tackled with increasingly encouraging results that may, in the not too remote future, be useful in vocational selection. (References to the work that has been done and good summaries are given by Allport (5). An unpublished thesis of M. J. Ream at the Carnegie Institute of Technology describes a valuable application of volitional and interest tests in the selection of salesmen.) Meanwhile systems of character analysis have flourished.

Most prominent of the systems of character analysis in the field of vocational selection have been those in which physiognomy and phrenology are the main ingredients, though some suggestions have also been made for the use of graphology (44), palmistry (93), etc. Doctor Katherine Blackford's method of character analysis (16) is far the most widely known and used. It is not greatly different in its essential nature from a number of other systems, as those of Merton (93), Balkin (9), McCormick (88), and others (41, 42). These systems make use of physical characteristics, for example, the form of profile, the complexion, texture of the skin, height of forehead, and so on, as indications by which to determine moral qualities and personality traits.

The adverse criticism of these methods divides into four charges: (1) The physical observations themselves are inaccurate and unreliable. (2) No scientifically valid evidence has been adduced in support of the alleged relations between the physical and mental characteristics. (3) The theoretical bases are unsound. (4) Estimations of characteristics from photographs under experimental conditions have shown highly unreliable results. Evidence on the first count is presented, for example, by Goring (52) by showing the inaccuracy of estimates of height of forehead. But this criticism is not applicable to many of the cues used by the character analyst. The second criticism is the only forcefully valid one. Objections on the ground that character analysis makes theoretical assumptions that are false (emphasized, for example,

by Hollingworth (59), Link (83), Adams (2, 3), Gowin (53), Payne (114)) give ground for justified scepticism but, after all, leave untouched the *practical* question as to whether correct character readings are made. Character analysts might conceivably be entirely wrong as to causal explanations and still have described actually diagnostic signs. A geocentric theory of the solar system makes no less valuable the prediction that the sun will rise each morning in the East. Experimental studies to date are likewise not at all rigidly convincing since the claims of character analysis have in no case been specifically put to the test. The reviewer has no thought of defending the popular systems of character analysis. It is maintained, however, that only by the same rigorously scientific processes used in examining one another's hypotheses, can scientific men satisfactorily deal with strange doctrines from without. Character analysts allege certain factual relationships to obtain. The disproof (and from a *practical* point of view disproof seems worth while) must consist in careful measurements and statistical correlations demonstrating the non-existence of those relationships.

Various bits of experimental and statistical research have presented presumptive evidence against character analysis methods, in addition, of course, to the anatomical and biological attack on the underlying assumptions. Schneider is cited by Hollingworth (59) as reporting that an examination of executives showed no relation of abilities with physical characteristics such as shape of head and hands. Pearson (116) found that the relationship was not close enough to make safe practical predictions from various head measurements, hair and eye color, etc., to intelligence. Experimental studies to determine the reliability of character trait estimates and intelligence estimates from photographs are reported by Hollingworth (59), Pintner (118), and Anderson (6). The general indication of all the studies is that judgments from photographs are markedly inaccurate and show very slight agreement with other measures of ability.

Kemble (72) is midway between the character analysts and psychologists. He emphasizes the value of an unanalyzed "size up" of the face but he thinks that this may valuably be supplemented by interpretations of specific muscular tensions of the face and the imprint of habitual tensions in facial lines. There is little in his proposed methods with which psychologists would quarrel on a priori grounds. His statistical evidence, however, is meagre and unconvincing.

*Psychological Tests.*¹—The most distinctively psychological contribution to vocational selection has been the development of tests. Tests as selective devices have been especially popular since the war—too popular perhaps. Before 1917 vocational tests were a hope, an interesting possibility, with, it is true, a few scattered pieces of research as a basis. The war gave a radically changed status to test work and made practicable much of what had been merely possible. After the armistice (as well as before in a very few instances) tests and testers invaded industry. Progress has been made slowly. Selective tests adapted to industry are still few; the movement is still immature; future possibilities are still uncertain. This is entirely healthy and normal if we recall the recency of the birth of scientific vocational selection. The danger—the aspect that is not healthy or promising—is the “over-selling” of tests, the tendency to dwell upon advantages and successes and gloss over defects and failures, the tendency to speak of selective tests as an accomplished fact and not as a valuable field for experiment. Perhaps that is the only way to induce the business world to try out test methods. Perhaps the goal of getting a foothold in industry justifies the means of gentlemanly exaggeration. But it is not science. And it is dangerous.

A few of the great number of discussions of tests for vocational selection which emphasize the need and desirability of using tests in employment (it is not implied that these are instances of “over-selling”) are those of Hollingworth (57), Ruml (134), Yerkes (179), Rossy (132), Viteles (168), Lamb (80), and Gardener (46). The last two report the successful use of tests in a New England silk mill. In some of the articles mentioned, as well as in others, the limitations of tests and the dangers in their unscientific use are emphasized, for example, in Scott (138), Ruml (134), Link (83), Watts (170). Similarly, the limitations upon the use of tests for vocational guidance have been discussed by several writers, including Ayres (8), Whipple (175), and Kitson (75). Very few writers (Thorndike is a notable exception) have considered the problem of coaching for tests—a problem that threatens to become increasingly important with the wider adoption of tests. A partial solution lies evidently in the use of equivalent alternative forms of tests.

¹ No descriptions of tests are included nor are the particular tests used by different investigators enumerated. The mere naming of tests has little value and space prevents their description, even were this deemed desirable.

Reports of actual accomplishment through the use of tests in vocational selection are not plentiful. Nor, for the most part, do they do more than convince one that here is a fruitful field for research. For a few occupations in a few companies, tests have been of demonstrated value. This gives promise. But the evidence is scant. In view of this condition it appears particularly unfortunate that a considerable body of scientific work on vocational tests carried forward by private companies and special research bureaus has never been published. The last report of the Committee on the Application of Psychological Tests and Rating Scales in Industry of the National Association of Corporation Training (108) states that 35 of the 172 companies questioned reported that they are using standardized tests of one kind or another for employment purposes. (The 172 companies are, of course, not a random sample of all companies.) Preceding reports have shown almost as many. Little, indeed, of this work has been scientifically evaluated and reported.

To further a healthy growth of tests in employment, there is need for as much convincing evidence as can be assembled. This means: (1) the publication of actual results from all sources, showing just how well the tests have operated in particular situations; (2) the non-suppression of negative results which are all too frequently doomed by the constant bias that tests *must* work, or reflect upon the experimenter; (3) the adoption of a more critical scientific attitude in place of the tone of special pleading apparent in part of the literature; (4) the use of more nearly justifiable statistical procedure and the more cautious interpretation of statistical findings.

Only the last of these four points merits an additional word. Passing over certain questionable procedures such as the use of correlations of attenuation when working with test predictions (used, for example, by Burt (25)) and the uncritical use of multiple correlation without mention of the large error to which it is subject (as used, for example, by Thurstone (165) and Bregman (21)), we wish to stress one frequent and specially favored statistical fallacy. In general terms, it is the interpreting of a relationship found in a selected group as true of a non-selected group. Specifically, it usually takes the form of establishing a correlation on a small group chosen in such a manner as to be non-representative of a larger group, and using the results as though they applied to the larger group. Scott (139), for example, in comparing test

scores with efficiency ratings for 26 employees in a silk mill, selects the men as follows: "Of all the men at work at the task it was desired to have tested about 10 who were extremely efficient, about 10 who were distinctly unfit for the task, and about 10 who were intermediate between these extremes." The correlation coefficient of firm rank with test rank was .88. But who can interpret the .88? Certainly it does not tell how reliably men can be selected for this work from a random sample of applicants. And that is what is wanted. In a later paper (137) Scott outlines this same procedure as a generally desirable method. It is desirable if our end be to obtain high coefficients. If the middle group in the example had been omitted and only the 10 at each extreme taken, the correlation might well have been .98. This is precisely what Bregman (21) has done. She selected from a group of department store workers those who were rated at the good and poor extremes and found single tests that correlated as high as .83 (using the "unlike signs method" moreover!). But on a vastly larger scale the Army Trade Test Division used the same sort of method, though in a less uncritical manner. These are but a few examples. The whole matter, on the positive side, may be summarized as follows: The conditions of try-out of a selective test and the nature of the group used should be as nearly as possible identical with the conditions and the group with regard to which the test is to be used in actual practice. The reason is obvious. A test may successfully separate the upper extreme of a group from the lower extreme (Scott's and Bregman's method) and still be of very slight value in separating the upper or lower extreme from the large intermediate assortment. Concretely, a test may separate extremely good salespersons from extremely poor ones and yet not be a test worth using. The real question is: How well does this test enable me to select good and poor salesmen from the general run of applicants. The answer can be obtained only by trying out the test on a group that is more or less nearly identical with a group of applicants.

A practice somewhat related to that just discussed is the interpreting of correlation coefficients without sufficient basis for interpretation. For test purposes, a coefficient of correlation is of use as an indicator of the reliability of prediction with the use of the test. It tells its story for a particular situation. But Hollingworth and Poffenberger (60), to cite only one example, fill a page and a half with tables of the correlations found with the

use of different tests for different occupations by many different investigators with entirely different groups of workers. The correlations range from .32 between ability as "specialized operators" and color naming test scores, to .96 between ability as "typewriters" and scores in letter substitution!

Tests of General Ability.—In discussing the vocational use of tests we may for convenience divide them into those of general ability (general intelligence, mental alertness, etc.), those of special ability as required by a particular job, and those of trade or occupational proficiency. The lines are not, of course, sharply drawn and in special instances the classification of a test as general or special, proficiency or ability, is almost purely arbitrary.

The history of tests of general ability or intelligence aside from their use in vocational selection need not concern us here. Before 1916 these tests had not been used to any considerable extent save in work with the abnormal and feeble-minded, and in a few special research studies most of which had no bearing at all upon vocational selection. One study, however, is to be mentioned. Woolley and Fischer (176) in 1914 published test results for some 800 adolescent children who were about to enter industrial employment. The tests used and the norms that were given aided in later developments though not of themselves significant for selection since no relations were presented between test scores and industrial success. Revisions of the Binet-Simon tests by Yerkes and Terman led to greatly increased application of test methods among normal people, especially in the schools. But the most significant advances in test methods as far as industry is concerned, which were made before the work in the Army, were the few attempts to construct fool-proof tests that could be administered to groups as well as to individuals, by relatively unskilled examiners, and that could be readily and objectively scored. Work along these lines had been developed by Scott, Thorndike, Thurstone, Otis, and others.

General ability tests took a tremendous leap forward with the cooperative effort of American psychologists to adapt test methods for use in the United States Army. The development and use of intelligence tests in the Army have been reported in a preliminary form by Yoakum and Yerkes (177); in an official volume edited by Yerkes (178), which contains a detailed history and description of the army test work and a wealth of well-ordered data showing test scores in relation to military rank, age, occupation, nativity, etc.; and in a large number of special articles among which may

be mentioned those of Yerkes (179, 180), Terman (159), Thorndike (163), Ruml (134), and articles in *Science* (183). The intelligence test ratings in the Army were used to furnish "a fairly reliable index of his (the soldier's) ability to learn, to think quickly and accurately, to analyze a situation, to maintain a state of mental alertness, and to comprehend and follow instructions". The value of the tests was checked by many special studies and comparisons. In actual practice the tests were used to select the mentally incompetent for special duty or discharge, to designate men whose superior intelligence indicated the desirability of advancement or special assignment, to provide a basis for balancing military units, and to make various special selections and assignments of men according to ability.

The primary suggestions of the Army work as regards industrial selection are along two lines: (1) Intelligence tests can be successfully used (a) to eliminate low-grade workers from industrial tasks where they would be dangerous or incompetent, and (b) to call to the attention of management, men of exceptional alertness. (2) Striking and significant differences were found in the range of test scores for different occupations. For example, medical officers, accountants, mechanical draftsmen had average ratings of B; clerks, telegraphers, etc., had averages of C+; machinists, carpenters, and other skilled tradesmen averaged C; laborers, barbers, etc., averaged C-. It seems likely that the alpha test especially favored persons with academic training and those having "pencil and paper" occupations. In any case, the significance of these differences for vocational selection is that they point to the possibility of establishing standards for different occupations. In and of themselves they are of no direct value in selection since the great amount of overlapping of scores from one occupation to another is no less striking than the fact that the averages are different.

The Army tests and a number of similar intelligence or mental alertness tests have been put into use in employment offices. Thirty-two of the 172 companies questioned by the National Association of Corporation Training (108) were using such tests. The tests are used to select individuals of the appropriate level of ability and to indicate ones who are too high or too low for the work considered. Standards are usually set, when set at all, by reference to the degree of efficiency on the job of individuals making different test scores. Kenagy (73) reports some interesting data

showing how intelligence test standards may be set with reference to labor turnover. It was found that the length of service of individuals above a certain test score was decidedly shorter than that of lower grade persons and similarly, below a certain dead line, length of service was low. The same sort of method is described in one of the Scott Company Bulletins (143). The use of intelligence tests has been confined in the main to office employees and salesmen. The use of these tests for definite occupations, we shall consider in our discussions of special tests for particular jobs since, although in form these tests are ones of general intelligence, in standardization and use they have been treated as tests for definite jobs.¹

Tests of Special Ability for Particular Work.—Special tests for particular jobs came into prominence as a possible method of vocational selection with the experiments of Münsterberg (99) though the way had been indicated by a few earlier pieces of work. A number of studies aiming to devise tests for special vocations have been made since these beginnings. There has also been some discussion of the technique of building these vocational tests. Münsterberg (99), Hollingworth (58, 59), Muscio (101), and Watts (170) are among those who have dealt most with the relations existing between the test and the job tested for, classifying the tests, for example, as Hollingworth does, into the four kinds: miniature representations of the job, samples of the job, analogues of the job, and tests chosen empirically with no obvious relation to the job. Hollingworth favors the last method. Typically, however, the empirical procedure begins with "hunches" that fall into one of the other classes. Watts (170, 172) argues vigorously against both the empirical method and the sample method, maintaining that "where intelligence as well as specific aptitude is necessary the analogous test is needed"—at least, he thinks, until we can form accurate psychographs for occupations.

Scott (137), Hollingworth (59), Link (83), reports of the National Association of Corporation Schools (106, 107, 108), and many other contributions have dealt with the methods of trying out and standardizing tests for particular jobs. Scott has outlined four methods of checking test results, as follows: (1) By comparison of test scores of present employees with rankings by executives;

¹ The reviewer is of the opinion that in our present state of ignorance regarding what a test tests, the part of wisdom is to call any test that proves valuable in selecting for a particular job, a test of the special abilities required by that job.

(2) By seeing if the scores of men of proved ability are high in comparison with those of applicants tested at the same time; (3) By comparison of test scores of applicants with their later accomplishments; (4) By comparison of a group of applicants with a group of employees who have succeeded. As to the distinction between (2) and (4), we are not clear. The first and third methods are the ones commonly used and advocated, if we include in (1) comparisons of scores with production records as well as with employers' estimates (and modify Scott's statement of the method of selecting the group). The consensus of opinion appears to be that, with proper safeguards, the third method is most desirable, since it most nearly measures precisely that relationship which is involved in predictions when the test is put to practical use.

Tests for particular jobs, as they are developed to date, may be considered in five groups: (1) tests for clerical and office workers; (2) tests for factory workers; (3) tests for salespersons; (4) tests for aviators and other military vocations; (5) miscellaneous special tests.

1. The tests for clerical workers described in the literature are in the main either intelligence tests or proficiency tests, although a few attempts have been made to devise special vocational tests to detect aptitude for particular lines of clerical work. The successful use of intelligence tests is reported by Scott and Hayes (142) though they present no actual data showing the value of the tests for selection. (Several bulletins of the Scott Company (143) contain such data.) Thurstone (165) advances the proposition that a special clerical test is, and should be, only a general intelligence test dressed in clerical language for the sake of the interest appeal. He describes a test of this sort and reports results obtained from 100 office workers who ranged from minor executives to young clerks. The correlation coefficient between grade of work (five grades were used) and test score (speed and accuracy combined by the use of multiple correlation) is .6. This tells little if anything about the value of the test for purposes of selecting from among applicants. It is significant, however, that the test is more closely related to grade of work than are age and schooling combined. Link (82, 83) is convinced that differential tests of special ability are required for the several kinds of clerical work, although he gives no evidence that better results may be obtained in this way than by the use of general tests. Both Link (83) and Scott and Hayes (142) show differences in average test scores

from one office group to another. The value of such averages is doubtful, since the overlapping of groups is so great as to preclude the possibility of making reliable individual predictions on the basis of the averages.

Link (83) reports that 935 clerks were tested in the employment office of an industrial firm, with a series of intelligence tests and tests for technique (card sorting, letter substitution, arithmetic, etc.) and follow-up reports obtained. Only 188 cases are discussed. These were clerks recommended on the basis of the tests and then followed up every month for three months. The tabulation given is:

Percentage of those called good by their superiors:

At the end of 1 mo. 75 per cent.

At the end of 2 mo. 89 per cent.

At the end of 3 mo. 92 per cent.

The size of these per cents means little since they depend upon the method of rating and the kindness of the superiors. The increases from month to month, however, are significant. Tests for typists, stenographers, and calculating machine operators are also reported by Link. More than 1,000 people were tested in the construction of these tests. Both tests of aptitude or ability and tests of proficiency were used. Results for typists and stenographers are not given, and only a few comptometer operators (in the calculating machine group) are reported upon. The tests in each instance, however, were deemed of sufficient value to be adopted for use in employment.

Among the earliest attempts to build tests for ability in special clerical occupations are those of Lahy (79), who tried out a variety of psycho-physical tests on typists and found a few of these tests that showed rather close agreement with typing ability, and those of Lough (87), who correlated scores on a substitution test with the abilities shown by commercial students in their several school studies and found fairly high correlations with typing, business correspondence, and stenography. A number of later studies have been made along more or less similar lines. Bills (12, 13), who used a series of tests on 139 students of comptometry and stenography in a technical night school, found a general intelligence test decidedly valuable in eliminating the failures and a special ability test in selecting the successful students. The tests were later found

to agree well with ratings of ability of two small groups of stenographers in a business concern. The method used by Bills is especially worthy of note. Scatter diagrams are plotted between scores on two tests, and the ratings of individuals according to the criterion are shown by different markings (as circles and crosses for successes and failures respectively) on the scatter sheet. It is then possible to draw two dead-lines and study the combined effect. This study also emphasizes the fact that different tests may be useful for predicting at different levels of ability. Bregman (21) reports the use of a long series of tests on a decidedly heterogeneous group of clerical workers in a department store, using only the extremes of the group. Some relationship of tests to ability is indicated but the correlation coefficients, as already pointed out, mean nothing.¹ Rogers (131) tried out 10 tests on 43 students of typing and stenography and compared these with typing tests during the training period and with instructor's estimates. Combinations of tests gave correlations of about .5 and .6. Jacques (64) reports some further work of Rogers on typists in a business firm. Several psychological tests (not proficiency tests) were found to correlate closely with the ability of 38 typists. No data are presented.

Cody (32) presents a long series of tests—all of them educational or proficiency tests—for office workers. The tests are for almost all the usual operations in a business office. They have been standardized among thousands of school students and office employees, but the author includes no statistical comparisons with criteria nor any measures at all save the average scores for different groups tested. Benge (11) developed a proficiency test for extenders and verifiers in a city gas company. Henderschott and Weakley (55) devised and tried out a test for billers and found fair agreement with estimated ability (though the fallacy of the selected group is again present). Marcus (89) developed a series of tests for Hollerith card punchers, which proved to be considerably more valuable in predicting ability than the civil service examination that had been in use, though even with the new tests the correlations coefficients do not appear to be large. Carney (28), experimenting with a large number of tests for selecting time clerks, obtained only a few fairly close cases of agreement between tests and competence on the job. Flanders (40) found no significant correlation between scores on an intelligence test by express

¹ Further aspects of this work are discussed under tests for salespeople.

clerks and their success at work. Wells (174) developed a test for file clerks but did not proceed with its standardization. Burt (25) used 10 tests on a group of general clerical workers but insufficient information is given to show what the obtained correlation coefficient of .56 means.

2. Only a few instances are reported in the literature of tests that were tried out for factory workers. Link's (83, 84) work in the Winchester Repeating Arms Co. is the outstanding example of thoroughgoing scientific test procedure in factory or workshop. Similar, but less extensive, research was later conducted in a Canadian rubber company by Burt (25).

Link (83, 84) describes in considerable detail the construction and standardization of special tests for five different occupational groups in the shop. In each instance a careful study is first made of the requirements of the work; then a series of tests is tried out; comparisons are made between test scores and some measure of working efficiency—production records where available and otherwise estimates by superiors. Sixteen tests, for example, were tried for shell inspectors, three of which proved especially diagnostic, giving correlations of .6 and .7 for a group of 52 inspectors. These tests were almost equally valuable for three other groups of inspectors doing highly similar work. Follow-up reports of applicants who had been tested also showed that the tests were operating effectively. The same tests were found to be very poor for selecting shell gaugers. A series of tests for assemblers of gun parts, tried with small groups of men and women assemblers, was found fairly satisfactory. A valuable test was devised and standardized for classifying a group of machine operators according to ability. And finally, tests for apprentice machinists were found which correlated very closely with estimates of ability for three small groups of apprentices.

Burt (25) used 32 different tests on several groups of workers. He does not mention the number of people in any of his groups. Correlations of tests with ratings of ability (using multiple correlation procedure and corrections for attenuation) ranged from .7 for "workers who hand out stock" to almost zero for "tire builders". With 400 workers in a silk mill, Otis (111) found no correlation between general intelligence test scores and ability at work. Thompson's very early work, reported by Taylor (156), consisted in the selection of inspectors of ball bearings by means of a reaction time test. Remarkably valuable practical results were obtained,

35 of the selected individuals doing the work that 120 unselected ones had been performing.¹

3. On the selection of salesmen even less test research has been reported than in the case of factory workers. Scott (139) published a little material on the selection of salesmen by tests as far back as 1915. He reports the use of an intelligence test including arithmetic, opposites, proverbs, directions, etc., with 10 travelling salesmen of a large tobacco company. The results are not unequivocal but show some agreement between test scores and sales records. A later series of tests for salesmen worked out under Scott's supervision is included in the *Aids in Selecting Salesmen*, of the Bureau of Salesmanship Research at Carnegie Institute of Technology (27). No results have been reported from the use of these tests. Further advances in tests for selecting salesmen, especially life insurance salesmen, have been made at Carnegie Institute of Technology but are not yet published.

In the field of retail salesmanship, also, certain tests have been developed at Carnegie Institute of Technology and tried out in department stores in Pittsburgh. The only published results concerning retail sales tests, however, are those of Oschrein-Bregman (21, 110). In the main study reported, very high *negative* correlations were gotten between scores on a variety of mental tests and ratings of ability of some 50 salespersons by their superiors. Only the direction and not the size of the coefficients is significant due to the way in which the groups were selected and the correlations calculated. A number of other statistical processes are used in this study which appear to the reviewer unwarranted and misleading. The same writer, moreover, had arrived at opposite conclusions in an earlier study (110) where she found high *positive* coefficients of correlation between the estimates of ability of 18 saleswomen and the scores on a series of tests. The earlier results are not reconsidered in the later study. Burt (26), reporting some results from data collected by Münsterberg, presents evidence that at least one of a number of tests tried on Harvard students and on several industrial groups is diagnostic of salesmanship ability. Hollingworth and Poffenberger (60) present some average scores in comparison with salaries for a group of 55 salesmen "engaged in selling all manner of commodities in all manner of ways. . .". They found some tendency for higher scores and higher salaries to go together.

¹ Other changes, however, were simultaneously introduced, making it impossible to say definitely what part of the gain was due to the tests.

4. Far more effort has gone into the study of aviation from the point of view of vocational selection than into any other single occupation, military or civil.¹ The principal results of American research are described in a number of articles (4, 34, 36, 56, 153, 154, 163, 181), and certain prominent features of European investigations are summarized by Dockeray and Isaacs (34) and touched upon by Stratton (153). Elaborate physical examinations and painstaking interviewing methods had been used in selecting aviation candidates from the beginning, but the predictions of future success had proved sufficiently unreliable to warrant intensive research on the problem by psychologists as well as by physiologists and medical men. Despite the tremendous difficulties involved in analyzing the abilities that enter into aviation and the corresponding difficulties of determining these capacities, considerable success was attained. Mental alertness tests and evaluations of a variety of personal information concerning the applicant were shown to be useful, on the one hand, and a number of psycho-physical tests were evolved to supplement these and get at the candidate's probable ability to fly and to endure such special conditions as partial asphyxiation. Correlation statistics presented by Henmon (56) and by Stratton, McComas, Coover, and Bagby (154) show rather low coefficients between scores on several tests and ratings of flying ability, but not so low as to be without promise. The latest report of research (154) in which a number of tests were used, both ones that had been found useful before and a number of new ones, concludes that: "The tests, as a whole, and some of them singly, are to some extent diagnostic. Their precise value, however, can be known only after trial under more favorable conditions."

Careful experimental studies were made of other military and naval tasks, one or two examples of which are briefly to be noted. Dodge, in a report edited by Yerkes (181), describes the methods he developed which proved extraordinarily valuable in the United States Navy for the selection and training of gun-pointers by means of specially constructed apparatus. He tells also of the tests devised for selecting recruits of relative fitness for plotting room service and of the comprehensive system of examinations of candidates for the Listeners' School in the Navy. In all these re-

¹ A great number of articles on aviation and the selection of aviators are available in both English and foreign languages. The present review omits all but a few references in this field since it is somewhat aside from the main line of vocational selection.

searches Dodge states that he followed "the general principle of using a replica of the actual task for test purposes instead of any presumptively correlated tasks". Myers (103) reports, without details, work along somewhat similar lines in the tests developed for the British Navy to select candidates for training in hydrophone listening. Ferree and Rand (38) present a few general conclusions from their experimental research on tests of vision for men in the look-out and signal service in the Navy.

5. Tests have been tried for a number of occupations other than those we have discussed. Münsterberg (99) very early devised his well-known tests for street car motormen, telephone operators, and ship officers. These tests, he states, "show a far-reaching correspondence" and "satisfactory agreement" with other measures of efficiency, but the data in support of the statement are not submitted. Goldman (51) summarizes this work of Münsterberg and indulges in some criticisms which are of doubtful value. Gerhardt (47) reports the use of some methods for selecting street car motormen in Dallas, including unstandardized psychological tests which are stated to have shown good agreement with the men's records. Burt (26) reports the results of 13 tests which Münsterberg had devised and tried out shortly before his death. The tests were given to six groups, collegiate and business, and attempts made to find tests particularly diagnostic of salesmanship ability, executive ability, and inventive ability. Since in the group of Harvard students these abilities were obtained as estimates of self, and since the business groups were very small, the findings are admitted to be tentative. At least one of the tests appears to be of value in predicting salesmanship and one for executive ability.

Seashore reported the beginning of his work on methods of determining musical ability in 1912 (144) and has made several valuable contributions at later dates. In his *Psychology of Musical Talent* (145), an elaborate array of tests arrived at through detailed laboratory experimentation is described for measuring the special abilities involved in music. No statistical data are presented in support of the tests. Watts (170) criticizes Seashore's method principally on the grounds that the complex interrelation of many factors precludes the possibility of their proper measurement and combination for use in vocational selection.

Terman (158) gives the results from the testing of 30 candidates for positions as policemen and firemen, with the use of the Stanford-

Binet examination and a series of pedagogical tests. As no criterion was available, the work has little significance for vocational selection. Freyd (45) describes a set of ingenious tests for journalistic aptitude and the results obtained when the tests were given to groups of students of journalism and to a few reporters. The group comparisons and the correlations with faculty ratings indicate that the test is effective. Jones (65) gave the Woolley test series to 22 boys studying telegraphy in a continuation school. A selected set of six tests was found that correlated to the extent of .8 with a ranking of ability made by the teacher of telegraphy at the end of an eight months' training course. The correlation between test scores and the ranking by employers (all worked for the same firm), of the boys' abilities as messengers, was almost zero. Thurstone (164) tried out eight tests on a group of 165 drafted men in a course in radio-telegraphy and compared the test scores with records of receiving speed. The highest correlation coefficient found was .48, with the use of an original "rhythm test". Intelligence tests correlated less closely. Kemble (72) describes a large number of tests for determining ability and classifying men—executives as well as men of lower grade—but he presents almost no evidence of the value of the tests, nor does he tell how they were standardized or what norms are to be used. The nature of his test work is indicated by his statement that "We have already arrived at the point where we can pick with rarely failing mathematical precision the right person for the right job. . .". In discussing the selection of executives, Gowin (53) cites other work of Kemble and also describes a minor experiment by Scott in which 19 executives of Cheney Bros. were rated by mental tests in almost exact correspondence with their ratings by the president of the company. Link (83), however, is convinced that it is "impossible to apply tests intelligently to executives in the higher and more specialized positions".

Tests of Trade Proficiency.—Discussions of trade tests are limited almost entirely to reports of the Army work, with a great deal of optimistic emphasis upon the desirability of extending this work to industry. Valuable descriptions of army trade test methods are contained in the Personnel System of the United States Army (117), Chapman (30), Bawden (10), Bingham (15), Reilly (128), Ruml (134), and in an article in the *PSYCHOLOGICAL BULLETIN* (182). Trade tests were used in the army to determine whether men were tradesmen in the lines in which they claimed skill, and

approximately their grade of proficiency. Oral and picture tests were used that consisted of carefully sifted and standardized sets of questions upon which the candidate could be rated. In some few trades, performance tests were available which required the candidate to perform some definitely specified task that was typical of his trade, a rating being assigned according to the excellence of the completed product and the time required. All the tests were tried out on groups of tradesmen in industry and evaluated by comparison with the known ability of these men.

Little has been added to the trade test work of the Army. Chapman and Toops (31) describe a written trade test that has the obvious advantages of a group test. Their statistical evidence is favorable though the nature of the group prevents its being considered conclusive. Chapman (30) discusses the adaptations of army trade tests used experimentally in the public employment bureaus. Brief standardized trade interviews were tried for a time, but for the practical needs of the situation unstandardized "selective trade interviews" were later adopted. Robinson (130) contributes a logical analysis of trade ability which makes explicit certain less evident features of the Army trade test method. Kelly (71) also discusses some basic considerations in connection with trade tests. Kornhauser and Ruml (78) point out some statistical difficulties of the Army trade test method and present a possible substitute. The use of trade tests in industry is discussed by a number of writers, including Bingham (15), Chapman (30), Ruml (134), Watson (169), Weaver (173). Link (83) views trade tests as an integral part of the interview and appears to give them rather scanty attention in comparison with that bestowed upon special ability tests. The actual use of trade tests in industry is scarcely mentioned in the literature. Proficiency tests for stenographers and typists have already been touched upon. Adaptations of trade tests to the selection of department store salespeople, according to their knowledge of merchandise, are reported in bulletins of the Scott Company (143). In regard to trade tests as well as in the case of ability tests, it appears that much of the work going forward has not been evaluated and reported, for the 1921 report of the National Association of Corporation Training (108) states that 26 companies use occupational tests and 25 use educational tests.

ANALYSIS OF THE QUALIFICATIONS OF WORKERS
ALREADY EMPLOYED

Vocational selection may be, and frequently is, concerned with selection from among employees already at work. For such selection, a prime requisite is a set of adequate records containing the information that has been obtained concerning the man during his period of employment. (Tests and other devices are, of course, useful in addition.) These records can conveniently be considered as of two kinds: (1) ratings of the individual's characteristics and efficiency, typically by his foreman or other executive, and (2) progress records including some measure of output, attendance, previous promotions, wage changes, etc.

Rating Scales.—Strangely enough, the rating scale first became widely known in the vocational world as an aid to interviewers in recording their judgments of new applicants (27), though its later history deals entirely with its more obvious use as a method for estimating characteristics of individuals with whom the rating official is acquainted. Rating scales, like most other personnel devices, sprang into prominence as a result of their widespread use in Army personnel work. The rating method which Scott and his associates had developed was adapted to Army needs and became the official method throughout the United States Army by which superior officers rated their subordinate officers. The Army system of rating is described at length in the Personnel System of the United States Army (117), in a bulletin by Reilly (128), in a report of the National Association of Corporation Schools (105), in an unsigned article (182), and in many special articles and books on personnel methods. The use of the Army rating scale involves the making of quantitative estimates of a man's ability in several more or less specific qualities. The ratings are assigned—and this is the most distinctive feature of the method—by a direct comparison of the individual to be rated with other individuals of known ability, whose names have been set down in advance as standards or inch marks on the rating yard stick. This man-to-man comparison scale has been widely adapted to industrial use. Paterson and Ruml (113) discuss certain improvements in the use of this scale and point out the values it has in industry. Rugg (133) finds that the scale probably did not reliably locate army officers within even the correct fifth of the scale. He suggests several conditions as prerequisite to the successful operation of the scale.

Many other forms of rating scale have been used, from the simple qualitative rating as Very Good, Good, Average, etc., and the rank order, to the recently developed graphic rating scale described by Scott and Hayes (142), in Scott Company bulletins (143), in the 1921 report of the National Association of Corporation Training (108), and in Hayes and Paterson (54). Good descriptions of a number of rating scales and their uses are contained in Kelly (67, 70) and Reilly (128). Miner (94) describes a method for rating an individual by placing a check mark at some point along a line to indicate how high in his group the man falls in each quality considered. Other scales have used descriptive adjectives or phrases to specify the several degrees of a given quality and provided that men should be rated by reference to these descriptive terms. The graphic scale combines these two features of checking on a line and having the standards specified by descriptive terms. This method has proved effective and extremely simple as compared, for example, with the Army method. The two methods are compared by Scott (63). No experimental study has been made, however, as to the relative merits of different rating systems save Kitson's (74) comparison of the Pearson method of rating intelligence with the Army method. He concludes that there is little real difference in the efficiency of the two.

Methods used in determining the qualities upon which executives should be rated and the weighting of these qualities are well described by Gowin (53) and similar methods as applied to men in training at an officers' training camp are discussed by Achilles and Achilles (1). Shelton (147) presents the results of a most interesting experiment in what he calls "mutual rating", where each individual in the entire personnel of a large office rated every other individual—subordinates, equals, and superiors—by means of a secret ballot.

Rating methods in general have been subjected to considerable criticism. Norsworthy (109), Cattell (29), Hollingworth (59), Cogan, Conklin, and Hollingworth (33), Rugg (133), and others have studied the validity of estimates of character traits. The judgments have been found to vary widely in reliability depending upon the qualities used, the individuals rated, and the persons submitting the ratings. Gillette (50) has argued against the entire assumption that ratings on unlike qualities can be summated, but this criticism apparently overlooks the fact that the numbers added are not estimates of unlike things but rather estimates of

the effect of unlike qualities upon a single linear variable, namely, the man's competence for a particular occupation. Gillette's criticisms of the choice of qualities and of the arbitrary weighting procedure are more valuable. Thorndike (160, 161) maintains, on the basis of statistical evidence, that a constant error runs through the ratings on separate qualities—a "halo" effect which spreads to all the separate qualities. He suggests, accordingly, that the rating official "should report the evidence, not a rating, and the rating should be given on the evidence, to each quality separately". Hollingworth (59) and others have pointed out some tendencies toward over-rating and under-rating of associates, depending upon the qualities in use. Simpson (149), Kelly (67), Link (83), and others have emphasized many of the limitations and more or less obvious difficulties of rating systems. In part of the literature criticizing rating scales there is evident that all too common tendency to condemn things on the ground that they fall sadly short of perfection, instead of coolly evaluating them in comparison with any reasonable alternative.

Two articles in particular have gone into the logic of rating methods and laid down valuable theoretical principles for the construction and utilization of scales. These are Thorndike's "Fundamental Theorems in Judging Men" (162) and Kelly's "Principles Underlying the Classification of Men" (71). Studies of this sort make vivid the intricacy of the problem and the consequent necessity for careful experimentation and statistical treatment by trained workers. The central problem of both papers is the question of weighting and combining a number of contributing elements in such a manner as to give the most adequate prediction of a man's success in one of several fields of work. The papers deal with considerations applicable not merely to rating scale technique but likewise to tests and other measurable elements in judging men's fitness. Thorndike concludes that in prophesying a man's fitness, we must assign weights to the traits according to "(1) their relation to fitness, (2) their partial constitution by common elements, and (3) any dependencies whereby one gains or loses in influence according to the amounts of the others which are present". The last necessity is especially disconcerting. It arises from the fact that qualities may have vastly different significance depending upon the extent to which some other quality or qualities are present. Thorndike suggests that informal intuitive judgments in the past have derived their strength, as compared

with formal scales, from the fact that they have allowed for the interdependence of traits. He thinks, perhaps too optimistically, that "sufficient insight and investigation should enable us to secure all the advantages of the impressionistic judgment (except its speed and convenience) without any of its defects". To date, however, we are still without evidence that the formal analytical rating method has given either better or worse results than could be obtained under the same circumstances by a single impressionistic judgment.

In view of the refinements of technique that have been developed for the obtaining and using of rating estimates and checking of results, and in view of the careful examinations of logical and psychological assumptions of rating methods, there would seem to be rather questionable support for the contention of the committee of the National Association of Corporation Schools (107) that "a person who is not a psychologist and has had no training in its technique may devise and use rating scales as successfully as a highly trained psychologist or even more so". Is rating scale technique, after all, less a concern of psychologists than a great part of the test work that has been called psychological?

Progress Records.—There is little that is of distinctly psychological interest in those devices of vocational selection that attempt to utilize all available information concerning the worker's productive efficiency, attendance record, earnings, etc. The psychological importance lies mainly in the emphasis upon the taking of a "clinical" view of the individual worker—considering him as a complete personality to be studied and evaluated from every angle—and in the stressing of the necessity for individual growth and advancement for the worker. These points of view, as well as detailed ways and means of keeping "living records" of employees with a view among other things to adequate provision for selection from among them, are well discussed by Scott and Hayes (142), Kelly (70), Link (83), Simons (148), and many others.

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REPORT
DEFINITIONS AND LIMITATIONS OF PSYCHOLOGICAL
TERMS, II.

PREPARED BY A COMMITTEE OF THE AMERICAN
PSYCHOLOGICAL ASSOCIATION

The first report of this committee, containing formulations of definitions (or delimitations) for 28 fundamental terms, was published in the *PSYCHOLOGICAL BULLETIN* for 1918, vol. 15, 89-95. The committee's work, suspended during the war, was resumed recently. Various causes have delayed the progress somewhat, but the following terms have been agreed upon.

As before, it was thought desirable to include alternative definitions wherever divergent usage appeared to justify them. The individual members of the committee do not personally accept all the definitions here given, but they believe that every definition included represents the usage of certain psychologists of good standing. The report is unanimous.

It is to be understood that while the American Psychological Association accepted the committee's report and authorized its publication, the Association is in no way responsible for the definitions, nor has it officially approved them.

VI

29. **Introspective Psychology:**

A systematic study of *mental phenomena* from the point of view of *introspection*.

30. **Analytic Psychology:**

A systematic study of *mental phenomena* in terms of their *elements*; sometimes contrasted with *genetic psychology*.

31. **Genetic Psychology:**

A systematic study of *mental phenomena* in terms of the origin and development of *mental life* in the *individual*, in the race, or in any part of the animal series.

32. **Structural Psychology:**

A systematic study of *mental phenomena* from the point of view of their (momentary) constitution.

33. Functional Psychology:

- a.* Psychology from the *biological* point of view, with emphasis on the significance of *mental life* as a process of adjustment rather than on its composition.
- b.* Psychology from the *dynamic* point of view, regarding *mental life* as developing a system of mental operations.

34. Dynamic Psychology:

- a.* A systematic interpretation of *mental phenomena* from the point of view of cause and effect.
- b.* A synonym for *functional psychology*.

35. Subjective Psychology:

- a.* Psychology restricted to the study of *mental phenomena* introspectively observable.

COMMENT: A term used to emphasize the exclusion of organic responses.

- b.* A synonym for *introspective psychology*.

36. Objective Psychology:

- a.* Psychology as concerned with *mental phenomena* expressed in the *behavior* of the organism to the exclusion of *introspective* data.

COMMENT: A synonym for *Behavior Psychology*.

- b.* Psychology as contemplating mental facts inferentially from without rather than introspectively from within.

VII

37. Activity (mental, psychical):

- a.* Any (mental, psychical) process, in the literal sense of a change.
- b.* Any such process regarded as originating in a subject.
- c.* Volition, will.

38. Action:

- a.* Movement made by an *individual* with conscious intent.
- b.* A synonym for *behavior*.

39. Act:

- a.* A consummated *action*.^a
- b.* A synonym for *action*.

40. Conation:

- a.* Activity seeking fulfillment; purposive activity in all its developments, such as volition, desire, aversion, impulse.
- b.* The mental state in which kinesthetic components predominate.

41. Conduct:

- a. The behavior of an individual as interpreted by other individuals in a common social relationship.

COMMENT: The term is primarily ethical.

- b. Behavior shaped by foresight.

VIII

42. Wish:

- a. An affective-conative tendency in respect to an imagined situation with the belief that the realization of the situation would satisfy a present want.
- b. Freudian: The *unconscious* urge of all living things to seek satisfaction (usually identified with, or illustrated by, the sex-impulse).

43. Disposition:

- a. General: Any organized tendency resulting from previous *experience*, individual or ancestral.
- b. Emotional: An organized tendency, the expression of which is predominantly an emotion or a sentiment.

COMMENT: Sometimes used as nearly equivalent to *temperament*.

44. Set:

Any organized mental condition or bodily structure which grounds a *disposition*.

45. Anlage:

The innate complex psychic organization modified by heredity which forms the basis of the individual's experience and development.

46. Mental Attitude:

- a. A stabilized *set* or *disposition*.
- b. (Bewusstseinslage.) An abbreviated but comprehensive *experience*, occurring principally in connection with affective, cognitive, and conative processes and at present incompletely analyzed.
- c. (Einstellung.) The specific mental *disposition* towards incoming experience whereby that experience is modified.
- d. Any mode of consciousness in which a self^a relates itself to its environment.

IX

47. Instinct:

- a. An organized mode of response determined by inherited neural *dispositions* that have been phylogenetically adapted to a specific type of environmental situation.

- b.* The inherited *dispositions* to coördinated activities which contribute to the accomplishment of a specific but not consciously predetermined end.

48. **Instinctive Behavior:**

Any complex response which is mainly determined (or so far as determined) by the inherited constitution.

49. **Instinctive:**

- a.* Pertaining to *instinct*.
- b.* Innate, *i.e.*, not individually acquired.

X

50. **Constellation:**

An organized group of mental phenomena larger than the inferior complexes out of which it is composed and smaller than the superior or total organization of mind.

51. **Mental Test:**

A standardized device for the purpose of measuring mental ability or of studying the results of mental operations in the individual.

MARY W. CALKINS,
KNIGHT DUNLAP,
H. N. GARDINER,
CHRISTIAN A. RUCKMICK,
HOWARD C. WARREN,
Chairman

A NOTE OF CORRECTION

In my review of Watt's *The Foundations of Music* (PSYCHOL. BULL., 1921, 18, 497-500) I have stated that "in applying his theory of fusion and interval to musical usage, Watt is forced to emphasize harmonic structure at the expense of simple melody." Through an inexcusable inadvertence I was led to support this view with a quotation which in its proper context was intended by the author to convey a contrary meaning. "Without harmony, which is the 'perpendicular' complement to the 'horizontal' functions of melody, we have a music which hardly deserves the name of art. It is merely primitive play, as it were" (p. 161). This extreme conclusion which appears to deny any system to the musical usage of the non-harmonic scales employed in the Orient is indicative of the influence a theory may exert over the facts it attempts to organize (p. 499). What the author actually states is that harmony has usually been put down as the one and only basis of true music, from which would follow that non-harmonic music "hardly deserves the name of art," but he adds that "an almost contrary thesis may be vigorously maintained."

While I greatly regret the carelessness with which my quotation was selected, and seek now to correct the impression I gave that Watt regards melody as secondary to harmony in musical evolution, I am unable to amend my conclusion that "many things like the pattern of pure melody seem to be neglected because they do not readily fall within the conception dominating the author's mind." It is against his conception of fusion and his derivation of the musical intervals therefrom that my argument was mainly directed and I am still unconvinced that Watt's foundations are adequate to explain melody.

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NOTES AND NEWS

PROFESSOR MADISON BENTLEY of the University of Illinois and Professor Herbert S. Langfeld of Harvard University will lecture during the summer at the University of California.

MISS FRANCES BOTKIN has been elected Instructor in Psychology at Smith College and Miss S. Myers has been promoted from assistant to instructor.

THE research chair of medical psychology in the University of Queensland, Brisbane, has been filled by the appointment of Dr. J. P. Lowson, university demonstrator in experimental psychology at the University of Cambridge.

THE second meeting of the informal group known as the "Boston Psychologists" was held at Wellesley College on Saturday, March 18. At the afternoon session questions of laboratory policy and the place of mental tests in systematic psychology were discussed. The dinner, at Tower Court, was followed by a toast to Professor Sanford of Clark University in recognition of his return to active service in psychology. The subject for consideration at the evening session was the status of the practising and consulting psychologist. Twenty-five psychologists from many of the New England colleges and universities attended the meetings. The first gathering of the group took place at Harvard University last November. The invitation to hold the next meeting in the autumn at Clark University was accepted.

PROFESSOR C. A. RUCKMICK, of Wellesley College, will have charge of the courses in psychology at the Summer School of the University of Kentucky this year.

AT the last meeting of the Ohio State Teachers' Association, a Department of Special Education was organized, with Dr. J. E. W. Wallin as President.

